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ABSTRACT

Research in social psychology will be more useful to society if researchers incorporate evaluation research into their inquiries. Evaluation research is interpreted to include overall assessment of a program or project as well as methodological procedures used to assess implementation and outcome of social and cultural action programs. Specifically, evaluation research will help social psychologists determine what problems are worth studying, which variables within a problem account for the greatest variance, and what level of intervention will be most effective. Also, evaluation research will benefit social psychologists by providing feedback regarding whether a theory successfully prescribes changes in a desired direction. The conclusion is that feedback gained from evaluation research can help social psychologists decide which areas of applied social research will have the most impact on social problems and social programs. (DB)

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High-Impact Applied Social Psychology[1]

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This paper is in the nature of a prognosis. I believe we know some of what ails social psychology, the "crisis" about which so much has been written in recent years; I believe evaluation research is one of the cures; and this paper will offer a rationale for why I believe that is so, and what changes in social psychology I suspect it will lead to.

Let me begin with a fable. Three social psychologists are out taking a walk and come upon a man rescuing a drowning person from a swift river. They watch as he no sooner resuscitates the victim, when the shouts of another drowning person are heard and the man leaps into the water to perform another rescue. After several more such rescues, the nearly exhausted man asks the first social psychologist for some help. The first social psychologist, a basic researcher, responds by saying, "Sorry, but I'm a scientist, not a direct service provider. I am so intrigued by your behavior, however, that I am going to start conducting research on altruism." The man appeals to the second social psychologist, an applied researcher, who responds by saying, "I'm sorry, but I am a scientist, not a direct service provider. I do, however, want to make a contribution to improving the human condition, so I am going to conduct research on more effective and efficient ways to pull drowning people from rivers." The man finally appeals to the third social psychologist, who responds by saying, "I, too, am a scientist, not a direct service provider. However, I read Saks' paper on impact, so I'm heading upstream to find out who's pushing all these people in." This little story illustrates a number of issues about question-asking strategies, the allocation of research time

and energy, and the benefit realized from the investment of those resources. These issues will be evident by the end of the paper, and may already be evident just through the fable.

To the basic-research scientist, a question is worth asking so long as the variables it addresses show variation and the scientist finds the question "interesting." To the applied scientist, additional criteria come into play. The one to which this paper speaks is that the relationships studied are important in proportion to the impact they have on the dependent variables that define the problem. That this is not a concern for basic research is evidenced by the fact that measures of association are infrequently computed and rarely reported. Never mind that the variance accounted for by an independent variable is less than one tenth of one percent; the effect is statistically significant, that supports the hypothesis, and we've got ourselves a publication. To the social psychologist engaged in application impact is of interest by definition. Not only must the effects be measured, not only must they have some effect, but also, the greater the magnitude of effect per unit of input resources, the better. Why is more "better?" Because the effects are more readily detected; because it means the manipulation or treatment or program is more cost-effective; and since change was the goal in the first place, the more of it the better. Or, with only slightly more complexity, we might say the greater the reduction in difference between where we are and where we choose to be, the better.

Evaluation research is the feedback loop that tells the applied scientist what kinds of manipulations and treatments and programs

maximize (or optimize) impact. And feedback is as important to shaping the behavior of the researcher as it is to shaping the behavior of other people (Powers, 1973). Program evaluation ties the applied researcher and the research to the goals of the intervention. As a result, it will change the behavior of the researchers--what they attend to, what they think about, what they do.

One change, for example, will be that independent variables will become more important to the researcher as they account for larger proportions of the variance in the dependent variables. Not only will measures of association become important, along with significance tests to test differences between treatment conditions, but social psychologists will begin to pay attention to the slopes of the regression lines as they search for the variables that produce the greatest change in the dependent variable per unit change in the independent variable (Blalock, 1964). That is to say, they will be looking for the variables that produce the biggest bang for the problem solving buck. Their theories and models will tend to include these variables and to ignore other variables. Such theories will come about not only because the interesting scientific questions will pertain to the most compelling available data, but good program planning will require understanding the most powerful relationships. As a result, the distinction between basic and applied research will become fuzzier than it already is. An important criterion for whether a theory is any good will be whether it successfully prescribes desired changes.

All of this behavior-shaping through feedback presumes some connection between applied social research, social programs, and impact upon social problems, so that there is some non-random information to be fed

back. The exact linkage among these elements is a matter of considerable theory and debate, including well over a dozen distinct alternative hypotheses (summarized in Abt, 1976). Although I am not a proponent of any particular model, my argument does assume that some of them are incorrect, namely, the hypothesis that no relationship exists (Banfield, 1974), and that any relationship is random and unsystematic. Somehow, applied social research has an impact upon social problems and evaluation research feeds those effects back to the researcher. That is my fundamental article of faith. The very existence of these hypotheses reflects yet another consequence of evaluation research: an increasing attention to meta-theories of planned social change, theories which seek to account for the nature of impact or the lack of it.

In terms of substantive research questions, an impact-sensitive applied social psychology will be alert to certain new, or at least newer, issues. Here is a sampling of the kinds of questions my prognosticating says will become salient. What problems are worth studying? Within a problem, which variables account for the greatest variance? Shall we treat a problem, or prevent it? What level of intervention will be most effective? What impacts are the criteria of ultimate interest? Let me give an example or two of each type of question.

> <u>What problems are worth studying</u>? For example, if we are interested in the just resolution of disputes, shall we study jury decision making, which accounts for fewer than 10% of cases litigated; or the behavior of legal services providers, who work on both jury and bench trials, the massive volume of negotiated

settlements, and the iceberg of legal problems that never become litigation (e.g., Cole, 1975; Saks and Hastie, 1978)? If we are interested in the elimination of poverty, shall we study the personalities and attitudes of the poor, or the socio-economic arrangements that tend to maintain their indigence (Allen, 1970)?

Within a problem, which variables account for the greatest variance? If we are interested in aggression, shall we ponder the complex subtleties of frustration or the ubiquitous power of reinforcement, modeling, and irritation as producers of aggression (Baron, 1972; Bandura, 1973; Berkowitz 1962)? If, against earlier advice, we find ourselves interested in influencing the decisions juries make, will it be more efficient for us to expend ourselves on the selection process or the persuasion process (Saks, 1976)?

Shall we treat a problem or prevent it? Efforts to reduce morbidity and mortality due to infectious diseases through identification and treatment, or even through prevention with vaccines, have had trivial success compared to such primary prevention as sanitation engineering, refrigeration engineering, and improved housing (which reduced the number of occupants per room) (Kass, 1971). An analogous set of options exists in the social problem called "mental health" (Brenner, 1973).

What level of intervention will be most effective? Here we must consider political or organizational structure in targeting interventions. Will the same expenditure of effort

have more impact in the community or at the level of state or national policy? What are the trade-offs, the multiplier effects, the dilution effects? Under this same heading we must also, however, consider the psychology of behavior change. For example, if we must consume less energy, shall we change peoples' attitudes and teach them techniques for saving energy, or develop "technological shortcuts" to social change that make the desired behavior automatic (Etzioni and Remp, 1973)?

What impacts are the criteria of ultimate interest? This is the stickiest question of all, but an impact perspective necessarily brings us to it. Shall we seek to maximize the length and quantity of life or the quality of it--comfort, functional capacity, autonomy? Given the goals of any project, where does the chain of impacts eventually lead? What are we trying to achieve? Given the coldly pragmatic scent of the impact perspective's first bloom, it is interesting that it also makes salient these questions of ultimate values, the kinds of things philosophers of ethics, theologians, and science fiction writers still occasionally confront, but which embarrasses most psychologists (for an exception, see Skinner, 1972). This perspective invites us to reconsider some major questions of existence.

Before we become too cosmic, let me bring us down to earth by reminding us that major social organizations will strongly resist high-impact social problem solving. Some people make their livings rescuing

drowning swimmers. Low-impact solutions mean there must be many service providers in order to meet the need. Cost-ineffectiveness is their bread and butter. They form associations partly to insure that unauthorized service providers do not cut into their territory, but also to protect the territory from shrinkage. If we cured all our ills in one fell swoop of high-impact problem solving, the armies of low-impact service-providing problem solvers would be transformed into the newest social problem. Nevertheless, my prognosis says it will happen; perhaps fortunately, it will happen relatively gradually.

It is not so much that evaluation research will facilitate the increased utilization of psychology. It is that evaluation research will cause psychology to become more utilizable. Evaluation research, at its best, will link psychology to the problems and questions of society, not merely to the curiosity of scientists. What social psychology will learn from this process will be more than a set of insightful lessons. It will be a veritable roadmap of how to be more useful to society, and not merely "interesting."

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